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Before Babel: Speculations on the Origins of Linguistic Diversity

Colin Renfrew

Recent research in historical linguistics suggests that groups or 'families' of languages may be classed together into larger language units or 'macrofamilies', for which some community of origin has been argued. The Afro-Asiatic macrofamily, for instance, which includes the Semitic and Berber languages as well as Ancient Egyptian and many languages of North and East Africa, is widely accepted among linguists. More controversial is the Nostratic macrofamily (including the Indo-European, the Altaic, the Uralic languages, etc.). The implications for prehistoric archaeology of the existence of such large linguistic units is examined. It is suggested that processes of agricultural dispersal may account for the widespread distribution of some of these macrofamilies.

And the whole earth was of one language and of one speech ... And the Lord said, Behold the people is one, they have all one language, and this they begin to do, and now nothing will be restrained from them, which they have imagined to do.

Go to, let us go down, and there confound their language, that they may not understand one another's speech. So the Lord scattered them abroad from thence upon the face of all the earth: and they left off to build the city. Therefore is the name of it called Babel: because the Lord did there confound the language of all the earth, and from thence did the Lord scatter them abroad upon the face of all the earth.

Genesis 11, 1-9

Inside or between languages, human communication equals translation. A study of translation is a study of language.

The fact that tens of thousands of different, mutually incomprehensible languages have been or are being spoken on our small planet is a graphic expression of the deeper-lying enigma of human individuality, of the bio-genetic and bio-social evidence that no two human beings are totally identical. The affair at Babel confirmed and externalized the never-ending task of the translator - it did not initiate it.

George Steiner, *After Babel*, 47-8

In its account of what must rank as the single most significant act of divine intervention since the Creation itself, the Book of Genesis offers a concise explanation of the phenomenon of linguistic diversity. Over the past two centuries the discipline of historical linguistics has given us clearer insights into the nature and scope of the diversity among the 10,000 or so different languages of the world. Prehistoric archaeology is likewise, using its own methods, developing an increasingly clear picture of the manner in which the world was peopled, and of subsequent developments in material culture. Yet it is far from clear that we have now any more satisfying account of the origins of such patterning as is found among the world's languages than is offered in Holy Writ.

In this article I should like to review some recent developments in historical linguistics which, if accepted, imply broad and hitherto unsuspected relationships between various human groups. These would carry radical new consequences for our understanding of the archaeological record. In the long run, in the face of such unexpected proposals, one must hope for a synthesis between three classes of evidence: historical linguistics, prehistoric archaeology, and molecular biology. There

are indications that such a synthesis may very soon become possible. It is the purpose of the present article to sketch some of the emerging outlines, and in so doing to lay particular emphasis upon the possible general significance, for the history of the world's languages, of processes of agricultural dispersal.

Languages and Language Families

For more than two hundred years linguists have classified the languages of the world into groups which are clearly seen to be related not only by similarities in vocabulary (lexicon), but by consistent patternings in the structures of words (morphology), and by regularities in sound shifts as one compares one related word with another (phonology). It is implicit within the 'comparative method', as the methodology which sustains historical linguistics has come to be called, that the related languages within a language family are to be conceived as descending from a common ancestral language. Each daughter language is then seen as the product of a specific series of phonological and morphological transformations upon the ancestral proto-language, with a vocabulary which is both depleted by word loss and augmented by innovations and by borrowings from adjacent languages.

It is evident, for instance, that the Romance languages (French, Italian, Spanish, Portuguese, Romanian etc.) are closely related in this way, and that this relationship would already be a clear one even if the common ancestral tongue (i.e. late Latin) had not been preserved for us in written records. In the same way, the Slavic languages (Russian, Bulgarian, Serbian, Polish, Czech etc.) may be seen as mutually related. So too the Celtic languages (Gaelic, Cornish, Breton, Welsh etc.).

Moreover such close-knit groups as these may be seen to form part of larger language families, whose relationships may be investigated in precisely the same way, using the comparative method.

Thus the Indo-European language family is defined to include the Romance, Slavic and Celtic as well as the Germanic languages, along with Albanian, Greek, Armenian, Indo-Iranian and so forth. To take a different example, just as well attested (and studied as early), the Uralic-Yukaghir languages form a large-scale family, analogous in that respect to the Indo-European, with several sub-families (e.g. the Finno-Ugric and Samoyedic families). The Polynesian languages form such another close-knit family, itself seen to form part of a larger grouping. The Semitic languages (including Arabic, Hebrew, and a whole series of dead languages such as Akkadian) form such another group.

All of this is familiar enough, and will be found more competently set out in any manual of historical linguistics (e.g. Anttila 1972; Hock 1986). Familiar also are the efforts which archaeologists and linguists have made over the years to find concrete explanations, in plausible historical terms, to account for these family groups. Such explanations in earlier decades were often expressed in terms of somewhat simplistic and arbitrary 'migrations'. Today the underlying arguments are more closely examined and debated (see Mallory 1989 & Renfrew 1987 for the Indo-European languages; Bellwood 1987 & Terrell 1988 for the Polynesian languages; Phillipson 1977 & 1985 for the Bantu languages). In each case it is generally assumed that the various members of the language family in question are all descended, as it were, from a common ancestral or proto-language. Only in a very few cases is such an ancestral language actually well documented, even for a small language family - Latin is one of the few such cases. For the larger family groups the common proto-language is invariably a theoretical entity, whose vocabulary, grammar, etc. are reconstructed, using the assumptions of the comparative method, by systematic comparisons among the assumed descendants, and by the development of the various transformational regularities or 'laws' already mentioned.

It is generally assumed, then, that new languages emerge through processes of divergence from a common ancestor, and that this process may be represented by means of the familiar family tree diagram. In explicitly formulating this model of change only a few years after the publication of *The Origin of Species*, Augustus Schleicher (1863) was directly influenced by Darwin's evolutionary thinking. Although processes of *convergence* such as word loan are recognized by linguists, and play a significant part in the discussion of creole languages, they have not yet been accorded a determining role in any well-established explanation for the origins of specific language families. The assumption of a common ancestor, of differentiation through divergence, and of dispersal through some kind of expansionary movement of people, prevails in the field of historical linguistics (and in the prehistoric archaeology based upon it) and has not been seriously questioned. The wider dispersal of the various languages of a language family thus is generally felt to involve processes of linguistic replacement (see Renfrew 1989, 117), in which unrelated predecessor languages in various regions come to be replaced by the family members in question. While some of these processes involve the movement of significant numbers of people, others (such as élite dominance, or the formation of a *lingua franca*) do not.

The debates which have hitherto taken place about the origins of specific language families have therefore centred upon the advocacy of alternative homelands for the hypothetical ancestral proto-language, upon differ-

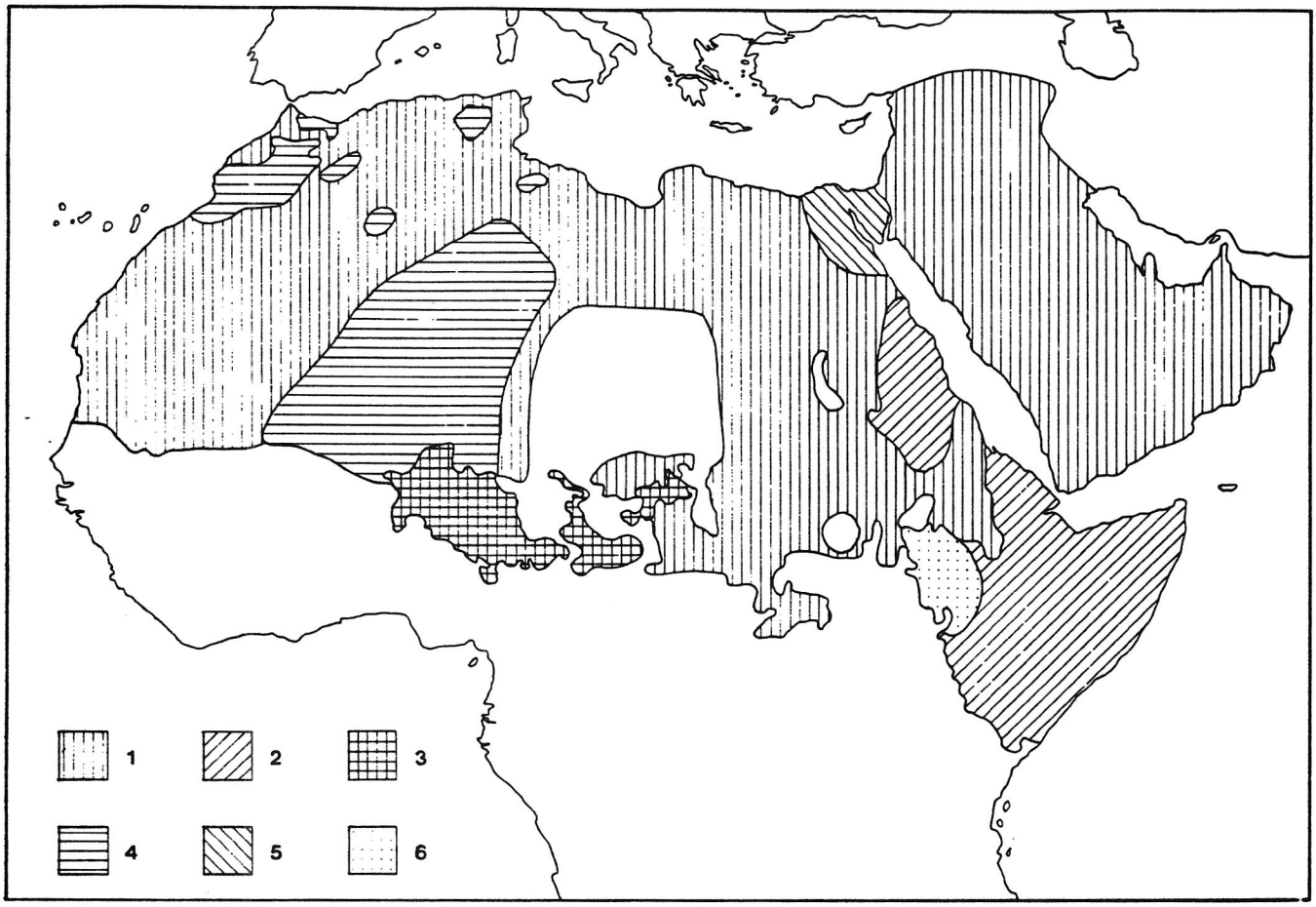


Figure 1. *The Afro-Asiatic languages. The present-day distribution of languages within the broader language groups which have themselves been classified together within the Afro-Asiatic macrofamily or language phylum. The constituent language families are as follows: 1. Semitic; 2. Cushitic; 3. Chadic; 4. Berber; 5. Ancient Egyptian; 4. Omotic. (Based on Ruhlen 1987, 86).*

ent chronologies, and on different sequences of migratory events. Inevitably the various hypotheses are difficult to test, dealing as they do with the prehistories of languages at a time well before they were ever written down. But the field nonetheless is a well-trodden one.

If this is the case, however, for the Indo-European or the Polynesian languages, it is certainly not so for some of the broader linguistic groupings which linguists have recently proposed. It is in relation to these that interesting new problems and possibilities arise.

The Afro-Asiatic (Hamito-Semitic) Languages

Linguists today are in agreement that most of the languages currently spoken in the Near East and in North Africa belong to a single large family or superfamily. The original grouping of the Hamito-Semitic languages has long been recognized (Diakonoff 1965; 1988), and it has now been enlarged so that this large family is seen to include, in addition to the Semitic and Berber languages and Ancient Egyptian, the Cushitic, Omotic and Chadic subfamilies (Ruhlen 1987, 86).

This large group thus includes nearly all of the languages of North Africa and of the Near East, with the exception of the languages of Iran and Kurdistan (Indo-European) and of modern Turkey (the Turkish languages, part of the larger Altaic family) (Fig. 1).

But what on earth would be the historical reality behind this remarkably extensive unity? Of course, for the use of Arabic in North Africa there is no problem. We know that Arabic came to be spoken there as a result of the Arab expansions of the seventh and eighth centuries AD - a remarkable instance of élite dominance. But by what historical processes would the Semitic languages and Berber be related? Or these to Ancient Egyptian? Or all of these to Omotic? Various hypotheses have been put forward in the past. Most of them, however, have the familiar property of assuming 'migrations' that would be linguistically convenient in allowing orderly dispersals from some hypothetical homeland. In most cases,

however, there is no concrete evidence to support such supposed migrations, and indeed no underlying explanation in demographic or social terms as to why such a sequence of events might have taken place at all. Such 'migrations' are all too frequently the arbitrary devices of the historian, or sometimes of the archaeologist, who find it convenient to push hypothetical groups around the map like pawns in a game of chess (although constrained by far fewer rules).

I have a suggestion to offer here, but will first introduce into the discussion a larger and still more puzzling linguistic concept.

The 'Nostratic' (Eurasiatic) Hypothesis

In the year 1903 the Danish linguist Holger Pedersen drew attention to similarities between a number of the language families of the Old World, including Semitic, Indo-European, Uralic, Altaic and Eskimo-Aleut (Pedersen 1931). These, he suggested, could be regarded as belonging to a larger linguistic unity which he proposed to call 'Nostratic', derived from the Latin *nostras* (gen. *nostratis*), 'our countryman'. This particular term today sounds excessively ethnocentric: 'our' is an inappropriate adjective when the language family might be studied by a Chinese speaker, or indeed by a Basque. For this reason Joseph Greenberg's 'Eurasiatic' might be preferable (Greenberg 1987, 332), or Dolgopolsky's term 'Boreic' (Dolgopolsky 1973).

The Nostratic concept was taken up with vigour by the Soviet scholar Vladislav Illich-Svitych (see Kaiser & Shevoroshkin 1988; Shevoroshkin & Markey 1986) and developed by him and by his colleague, now based in Israel, Aharon Dolgopolsky (Dolgopolsky 1989). Sadly, Illich-Svitych was killed in a road accident in 1966, and his work and that of his colleagues and successors is only now becoming widely known in the west (Bulatova 1989).

Illich-Svitych (1971-1984; 1989; 1990) was able to show, following the direction indicated by Pedersen, that it was possible to go a significant step further (i.e. earlier) in the comparative linguistics of Europe and Western Asia than had hitherto been attempted. He employed what, broadly, was that same methodology, the comparative method, which had long been used. Given a number of related languages within a language family, it aimed to reconstruct some of the vocabulary and other features of the hypothetical proto-language, and to show how the daughter languages could be seen to descend from the parent by well-defined phonological transformations. Illich-Svitych was able to show that various language families, each with its hypothetical proto-language, could be compared using these methods in much the same way as if they had been simply single languages within an individual family. The concept which emerged, as glimpsed by Pedersen, was of a much larger superfamily or macrofamily or linguistic phylum, embracing a whole series of lesser families. At its heart, at some very early time (set by many Nostratic scholars as sometime before 15,000 BC) lies the notion of the Nostratic proto-language, a higher level proto-language, the common ancestor of all the proto-languages within the group.

The language families which he recognized as having a common ancestral family relationship in this way are:

the Indo-European language family
the Afro-Asiatic family
the Dravidian languages
the Altaic language family
the Kartvelian (South Caucasian) languages
the Uralic-Yukaghir language family

This offers an astonishing and breathtaking perspective - a vast linguistic panorama (Fig. 2). The present extent of the Indo-European family, of course, covers most of Europe, plus Iran, Pakistan and much of India (not to mention the products of later colonization in the Antipodes and the Americas). We have already seen the great extent of the Afro-Asiatic family. The Dravidian languages, along with the Indo-European, effectively cover the whole of the Indian subcontinent (with the exception of the Munda languages (north-central India), Sino-Tibetan (near the Burmese frontier and in the Himalayas), and the two small isolates Nahali and Burushaski). Then if the Uralic languages (including the Finno-Ugric) are taken into account, and the Altaic (including the Turkic), plus the languages of the southern Caucasus, then all of Europe and the western half of Asia are included (always excepting Basque and the North Caucasian languages).

It is difficult for anyone other than a competent linguist to comment upon this remarkable hypothesis. But it should be noted that Illich-Svitych's lexicon of the proto-Nostratic language contained some 600 roots, from which he identified derivatives in many of the languages of the family. Moreover his work and that of Dolgopolsky (who has now compiled a dictionary of more than 1,500 roots) conforms to the traditions of the comparative method in giving clear rules for phonological changes from the proto-language (Dolgopolsky 1989; Kaiser & Shevoroshkin 1988, 316-28). Grammatical morphemes (suffixes and prefixes) of the daughter languages have been explained as well.

It is not, therefore, premature to take this work seriously, even if it must certainly be considered hypothetical until more widely reviewed in linguistic circles.

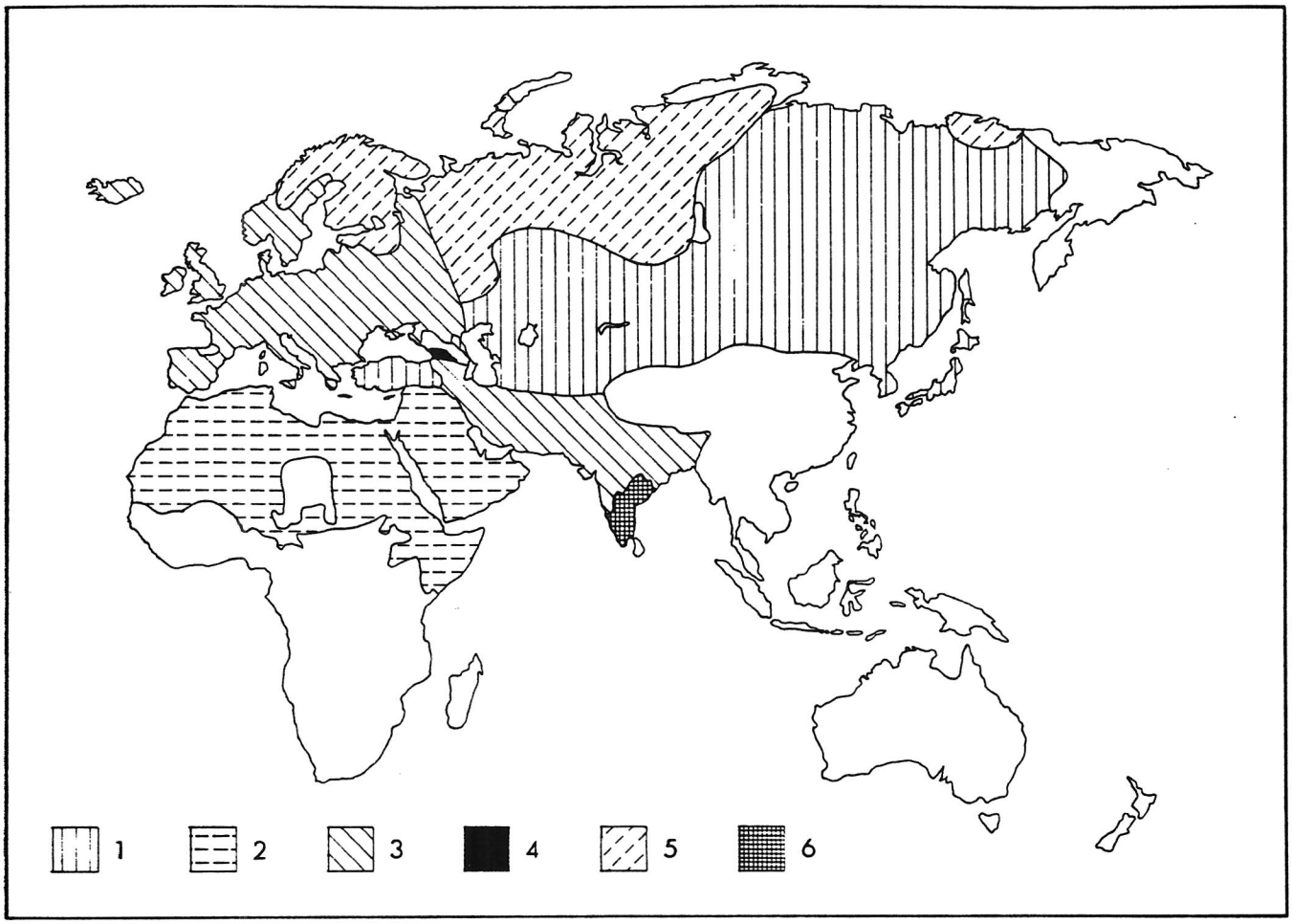


Figure 2. *The Nostratic macrofamily. The present-day distribution of the language groups which were classified by Illich-Svitych within the Nostratic macrofamily. The constituent language families are: 1. Altaic; 2. Afro-Asiatic; 3. Indo-European; 4. South-Caucasian; 5. Uralic; 6. Dravidian. (The effects of European colonial expansion since the fifteenth century AD are not indicated.)*

Even if the Nostratic hypothesis is ultimately seen as flawed, the exercise of taking it seriously will, I believe, throw up a number of methodological problems which are of real interest in their own right. And if it is later more widely accepted, then it will carry with it significant implications for our understanding of the prehistory of Europe and western Asia.

What, then, if the Nostratic hypothesis were right? What if all these languages were indeed related by some familial affinity? What are the implications, if we should be thinking of an early, proto-Nostratic language, spoken perhaps around 15,000 BC in some area within Europe and western Asia, from which all these later languages would in some sense be descended? How can the prehistoric archaeologist, already perhaps daunted by the complexities of the Indo-European problem, begin to cope with the vast new perspectives which might then be implied?

Seen from a very general standpoint, only two classes of explanation seem at all likely. The first, inevitably, goes back to the traditional approach, which may be summarized as 'Hunt the Homeland'. In other words, we would be thinking in terms of the conventional view of dispersal and divergence. No doubt many specific possibilities will be proposed. I will outline below one which seems to me the most plausible, in terms of agricultural dispersals.

First, however, it will be interesting to seek an explanation in the second explanatory class: one that does not rely heavily on episodes of linguistic replacement. What if the Nostratic family were, to echo Sir John Myres' expression for the Greek language (Myres 1930, 358) 'ever in process of becoming'? (Or if not quite 'ever', at least for an exceedingly long time.) Here we are talking of a very long time scale, a Palaeolithic time scale. This we might call the Gravettian Proposal.

The Gravettian Proposal

As an exercise it is perhaps instructive to try to construct an explanation which is not simply the conventional migrationist one, based upon dispersals from some putative homeland (*Urheimat*) by the *Urvolk*, followed by the usual processes of linguistic divergence. This is essentially the Schleicher model, which as we have noted, is an underlying assumption (not a conclusion) of the comparative method, and one without which that method is quite unable to operate. Alternatively we might try to devise something closer to the convergence approach, first proposed by Trubetzkoy (1939) for the Indo-European languages. There is no reason why such thinking should not be applicable to this larger (and earlier) linguistic grouping.

The notion here would be that for some considerable time before the emergence of the Nostratic proto-language there would have been various language speakers over much of the area in question who would have been interacting in several ways. Partly as a result of these interactions a process of linguistic convergence would develop by means of which the various languages spoken would approach each other in various respects and come to share more features than they had previously done.

We would be speaking here of hunter-gatherer communities, of course, not agricultural ones. These would be in the main mobile communities. It has often been noted that the range of movement of mobile hunter-gatherers is very much greater than that for sedentary farmers. Moreover, hunter-gatherers need to exchange information with their neighbours in order successfully to exploit the environment. It might be argued, therefore, that the scale (in terms of area) of language groups would naturally be larger among hunter-gatherers than among farmers, and the extent of interaction more considerable.

On the Trubetzkoy approach it would not be necessary for the languages spoken in the area in question to be initially related in the first place. They would gradually take on a shared Nostratic character as a result of interaction and hence of convergence. (It should be noted that Trubetzkoy did not recognize the need for a *single* hypothetical Indo-European proto-language: the Indo-European features of the family members came about through convergence, not through common ancestry.) Application of the Trubetzkoy approach to the Nostratic case would suggest that the different proto-languages underwent a process of convergence so that they came to share various features in common. The hypothesis of a common Nostratic proto-language might be unnecessary.

The Gravettian proposal differs a little from this view, although it still foresees a long period of *in situ* evolution. It is the case that during the Upper Palaeolithic period, around 25,000 BC, there are striking resemblances and uniformities in much of Europe and western Asia, clearly reflected in the material culture. This is true not only in the lithic industries, which from Dordogne to the Urals are termed 'Gravettian' (after the type site in the Périgord). It holds also for another striking class of artefact, the so-called 'Venus figurines'. From the Vézère valley in Dordogne in the west across to Dolni Vestonice in Czechoslovakia and so east to Kostienki in the Ukraine, very similar artefacts are found (Fig. 3). As Grahame Clark (1977, 105) observed: 'They suggest that a broad community existed in the psychic as well as the technical fields between eastern and western Europe'.

Already the question of the scale of the largest social units in Palaeolithic times has been well discussed by Wobst (1976), who considers the mating networks operating in hunter-gatherer societies and the possible expression of the interactions which sustained them in various symbolic items. He points out, moreover, that among recent hunter-gatherers the mating network is equivalent in scale and membership to the dialectical tribe: there is the clear possibility of a linguistic correlation. Gamble (1982) has drawn attention specifically to the female figurines of the Gravettian as a possible indication of an open social network of much the kind discussed by Wobst. The significance of this large area of symbolic uniformity in Upper Palaeolithic times may thus have considerable relevance for linguistic questions.

The Gravettian proposal, then, is that there was already in Upper Palaeolithic times a degree of underlying cultural and linguistic unity over much of the area where Nostratic languages were later spoken. This unity is then the basic underlying explanation for the resemblances seen among the various Nostratic languages. Other aspects of the Nostratic languages might indeed be the result of convergence of the kind suggested by Trubetzkoy. But the Gravettian proposal sets very much earlier, as far back as 25,000 BC, many of the elements (or at least their origin) which compose the Nostratic unity.

By taking the story back to the very early days of our species we are of course bringing ourselves close to the period of initial human dispersals out of Africa. The model here is thus not so much one of linguistic replacement as of initial colonization, with subsequent processes of convergence as well as of divergence. Of course expressed in this summary way the proposal could carry little conviction: it needs elaboration. For most linguists the proposed date would, of course, be seen as far too early. But the intention here is simply to indicate an interesting case of widespread unity observed in the

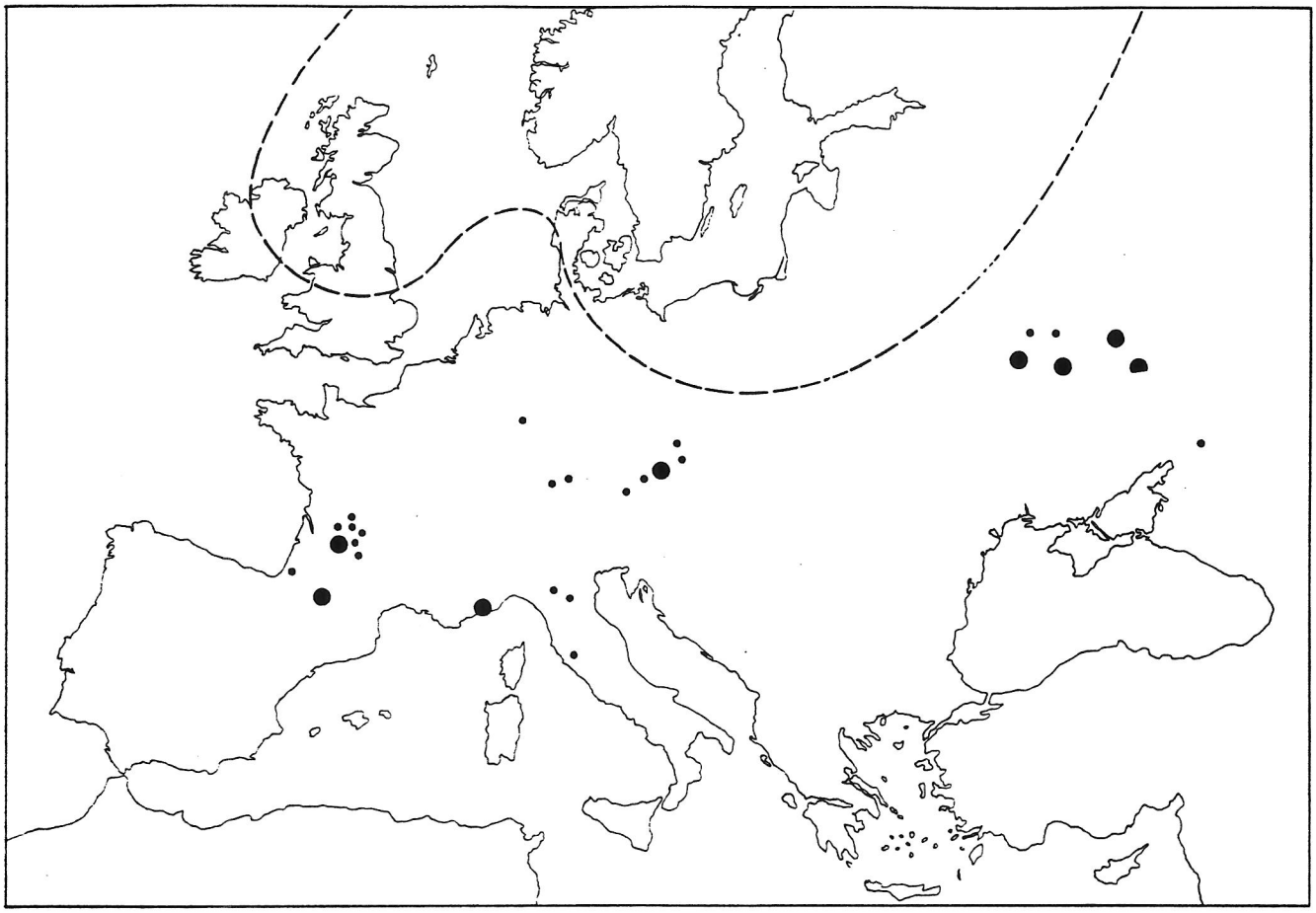


Figure 3. *The distribution of Venus figurines c. 23,000 to c. 21,000 B.C. The larger dots indicate a number of figurines from one site. The broken line indicates the maximum extension of the ice sheets at c.16,000 BC. (after Gamble 1982).*

archaeological record for comparison with the later linguistic evidence for relationships on a broad geographical scale. The conventional linguistic argument, that the well-established archaeological date for this widespread distribution of artefacts is too early, may well be valid. But the underlying grounds for the relevant linguistic chronology have never been made entirely clear, and would need to be set out more coherently before the objection could be accepted as definitive. It should be noted, however, that the Trubetzkoy convergence model is one which arouses great hostility among the majority of linguists. Dolgopolsky comments as follows:

> Trubetzkoy's hypothesis of the Indo-European language as a result of convergence cannot be accepted (and is not accepted by any trained comparative linguist). The matter is that contacts and mutual influence cannot create a common morphology and common basic lexicon. In the whole known history of languages no such convergent origin of a language has been reported. Today we know that a *Sprachbund* ('league of languages') resulting from contacts and mutual cultural influence (such as the modern West European Sprachbund, including English, French, German, etc.) is clearly distinguishable from a language family. Therefore the Gravettian proposal is hardly acceptable even as an alternative. Such things never happened in the history of the known languages.
>
> In spite of the mighty influence of Russian on all languages of the USSR, there is no Soviet family of languages. Vietnamese and Thai, though influenced (lexically and typologically) by Chinese remain unrelated to Chinese. So too, in spite of French, Latin and Greek components of its vocabulary, English is a Germanic language. Persian (in spite of a mighty Arabic influence) is Iranian and not Semitic. No linguistic family has been born from convergence, as far as we can judge from the extant evidence. (Dolgopolsky, pers.comm.)

This, then, rather than the issue of very great time depth, must be seen as the weightiest objection to the Gravettian proposal as formulated here. I am grateful to Dr

Dolgopolsky for this very clear critique of the convergence approach: he points out that Trubetzkoy himself never took his own proposal very seriously, and never returned to it or elaborated it in his further work. Certainly it would seem that the overwhelming majority of linguists prefer a divergence approach.

The alternative view, that of language dispersal and divergence, in conformity with the usual assumptions of the comparative method, will now be explored.

In doing so, we should note that the Gravettian cultural phenomenon might conceivably find correlation with another, still rather tenuous linguistic entity, whose origins must precede the dispersal of a farming economy. Starostin (1989) has suggested a relationship between the Sino-Tibetan and North Caucasian language groups, a linkage for which any historical reality is at present far from evident. (The grouping also contains the Na-Dene languages of North America, but for these a relatively recent, post-glacial origin has in any case been argued.) Other languages for which a North Caucasian affinity has been argued include Hurrian, Hattic, Etruscan (Oren & Starostin 1990) and Basque (Ruhlen 1991, 16). It would be possible, although perhaps still premature, to suggest a very widespread and early distribution of proto-Dene-Caucasian languages embracing much of Europe and northern and central Asia, itself overlain by a subsequent (and hypothetical) Nostratic dispersal from the agricultural nuclear areas of the Near East and Anatolia, which will be argued in the next section. Such an underlying unity could well be situated in the Upper Palaeolithic of Europe and north-central Asia.

Language and Agricultural Dispersal

The role of agriculture in language dispersal has been advocated in particular contexts by a number of scholars (notably Shnirelman 1989). In each case a nuclear area is defined within which a specific range of wild plants (and sometimes animals) is located. These proved amenable to domestication, and their domestication did indeed in due course take place. In many instances the farming 'package' of plants (and where appropriate of animals also), along with the appropriate exploitative techniques, proved an expansive one. The expansive properties of the new farming economy in each case may be seen as dependent upon three factors. The first was that the plants (and animals) should be suitable for transplantation into new ecological niches, at any rate when they were sustained by the appropriate exploitative technology from the accompanying human population. This technology will have involved propagation (i.e. seeding, or controlled breeding), protected growth (for instance by weeding and manuring, or with animals by controlled feeding, for instance by transhumance), and organized harvesting (or culling). Secondly, the expansive nature of the new economy depended upon an increased birth rate and reduced rate of human infant mortality, and sometimes on an increased post-infantile life expectancy, associated with aspects of the new subsistence regime. The most significant of these was, in most cases, the sedentary life which farming allowed. Thirdly, the new economy will have permitted a much greater intensity of production, as measured in terms of food (calories) produced per unit area per year. Agricultural economies, even of a simple nature, are characteristically about one hundred times more productive in this sense than mobile hunter-gatherer economies, or at least they have the capacity so to be. Only in those hunter-gatherer economies which rely on some particularly productive marine or riverine resources does a sedentary economy with consequent increase in population density become possible. (This was indeed a property used by Binford in his 'Post-Pleistocene Adaptations' article (Binford 1968) to explain the origins of farming economies.) Thus the 'carrying capacity' of the new farming economy will have been much higher than that of the preceding hunter-gatherer one. These features were already recognized by Gordon Childe (1936) in his early formulation of the concept of the 'Neolithic Revolution'.

These factors, taken together, led at least in favourable cases to the expansion of the new economy beyond the limits of the nuclear area where the potential plant and animal domesticates were indigenous. Various mechanisms of propagation may be proposed. At one extreme is the 'wave of advance' model of Ammerman & Cavalli-Sforza (1973), a model of demic diffusion, in which the expanding human population carries the new farming economy with it in a long series of very small steps. For individual farmers the direction of movement would be random, and the distance no more than some 20 to 30 km, but the overall effect would be the expansion both of the farming economy and of the descendants of the human population of the nuclear area until the limits of possible expansion were reached, either at the extremes of the relevant landmass, or at the limits of the ecological zones with appropriate temperature and rainfall. At the other extreme one may imagine a process of cultural diffusion, whereby the elements of the new economy, including the necessary plant (and animal) resources, would be transmitted from one group to the next, with a consequent increase in population density but without migratory movement and thus without significant change in the genetic composition of the human population.

In both such cases it is to be predicted that very widespread linguistic replacement may occur. Indeed,

agricultural dispersal is a special (and important) case of the demography/subsistence model proposed elsewhere (Renfrew 1989, 117) as the first and perhaps the most significant of the various models for linguistic replacement. In these circumstances, we may expect the language of the farmers, in many instances, to replace that of the pre-existing hunter-gatherers whose habitual hunting grounds formed part of the area exploited by the new farming economy.

It is of course entirely possible that the hunter-gatherer economy will survive for a long period side-by-side with the new farming economy (Zvelebil & Zvelebil 1988), and indeed that the pre-existing language may survive for some time alongside its potential successor. There is no necessity that linguistic replacement need accompany agricultural dispersal. But even when the original human population survives and then increases as a result of the adoption of the new economy, it is very possible that it will, in time, come to adopt the imported language associated with the new economy and with the new ideologies which may accompany it (Sherratt 1990; Hodder 1990). Cases of language replacement among hunter-gatherers through contact with farmers, but without the adoption of a farming economy, have been usefully discussed by Ehret (1988).

A model of approximately this kind has been advocated by a number of workers for specific cases of linguistic replacement. Peter Bellwood, in a number of papers (1985; 1987; 1989; n.d.) has discussed the issue as it relates to the Austric family of south-east Asia. The same model of agricultural dispersal applies, of course, to the specific case of the Polynesian languages, where it has been well discussed by a number of scholars in addition to Bellwood, including Terrell (1988), Kirch (1986) and Kirch & Green (1987). Since the Polynesian islands were uninhabited before the dispersal in question, this was a case of initial colonization rather than of linguistic replacement. But the model of agricultural dispersal (along with the wave of advance model, appropriately modified to the insular environment) applies.

Comparable arguments have been made for the Bantu languages of Africa, which fall within the Niger-Kordofanian family of Greenberg (1963).

I have myself advanced a particular instance of this model in suggesting a dispersal from Anatolia to Europe of a proto-Indo-European language or languages in association with the coming of farming to Europe (Renfrew 1987). It is now well-attested that the relevant economy based upon wheat, barley, sheep and goat was established at sites in the Plain of Konya (such as Aceramic Hacilar and Çatal Hüyük) and further east, in the seventh millennium BC. This area may be regarded as one lobe of the Near Eastern nuclear area (Zohary & Hopf 1988). There is little doubt, likewise, that farming came thence to Europe by a process of agricultural dispersal much like that outlined above. Whether a 'wave of advance' model is appropriate, implying a change of population, or rather one of cultural diffusion, with the local adoption of a farming economy, as advocated for northern and western Europe by Zvelebil & Zvelebil (1988), is not yet entirely clear. But in either case it may be hypothesized that the consequence of the farming dispersal was the ultimate displacement by the incoming proto-Indo-European language of the indigenous hunter-gatherer languages of large areas of Europe.

It is at this point that we can return to the Nostratic hypothesis, and make a more concrete proposal for the origin of the early distribution of the Nostratic languages, in terms of agricultural dispersal. This possibility has already been foreseen or briefly outlined by Cavalli-Sforza (in press), De Laubenfels (1981), Krantz (1988), Sherratt & Sherratt (1988), Renfrew (1989, 136) and Militarev & Shnirelman (1988). It is proposed that precisely the same arguments which allow one to expect an agricultural dispersal with accompanying linguistic replacements from Anatolia to Europe are likely to be applicable to other lobes of the western Asiatic area of agricultural origins.

The underlying argument is well brought out in a simplifying diagram by Sherratt & Sherratt (1988) (Fig. 4). The Anatolian lobe of the nuclear area is represented by the figure 1 on this diagram, with the subsequent dispersal to Europe represented by the figure 1'. The figures 3 and 4 refer to the Levant and south Zagros components of the nuclear area, while the figure 2 in this case may be taken to refer to the north Zagros/east Anatolian area, and possibly Turkmenia also, if this be taken to be within the nuclear area of cereal domestication (see Masson & Sarianidi 1972).

Would it not be attractive to account for the distribution of the Nostratic languages in this way? A hypothetical map is set out here (Fig. 5). It is, however, important to appreciate the degree to which it remains hypothetical. Only in the case of Anatolia-to-Europe is there general agreement that there was indeed an agricultural dispersal of this kind, whatever may be the reservations about its linguistic implications. For the other lobes, which certainly may be regarded as lying within a nuclear farming area (Zohary & Hopf 1988), the subsequent farming expansion is not yet so well documented.

The hypothesis, then, is that within a nuclear farming area adjacent to the 'fertile crescent' - in the Levant, in the western Zagros foothills, in east-central Anatolia, and possibly in Turkmenia - a new farming economy came into full development between *c.*10,000 and 8000

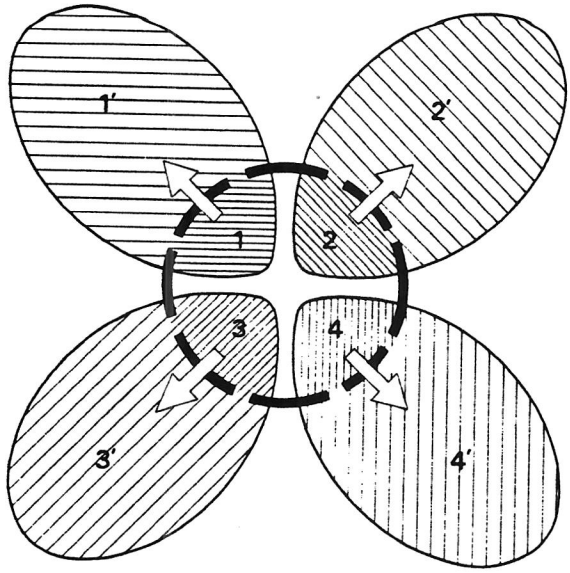


Figure 4. *Farming origins and language dispersal. When a transition to primary farming occurs within an area with some linguistic diversity (shown within the broken circle) the consequence of the ensuing agricultural dispersal is likely to be linguistic replacement in adjoining areas. The lobes represent the areas occupied by the subsequent language families derived from the corresponding proto-languages. Such processes may underlie the distribution of several of the world's linguistic macrofamilies. (After Sherratt & Sherratt 1988).*

BC. It was based principally upon wheat (initially einkorn and emmer) and on barley, supplemented by pulse crops, and on the husbandry of sheep and goat. The emergence of this economy could only take place within the habitat areas of the principal species involved, notably wheat and barley. The primary region was therefore a restricted one (Zohary & Hopf 1988).

This proved to be an expansive economy in the manner discussed earlier. Whatever the appropriate model for its mechanism, the dispersal of the plant and animal species did indeed take place. It is here proposed that the languages originally spoken within each relevant segment of the nuclear area were in consequence propagated beyond it, as indicated by the diagram (Fig. 4).

As already discussed, and argued in more detail elsewhere (Renfrew 1987; Gamkrelidze & Ivanov 1984; Dolgopolsky 1975; 1987; Shevoroshkin 1987) it has been suggested that a proto-Indo-European language, or languages, was spoken at this time within the east-central Anatolian lobe of the nuclear area, and that they were propagated to Greece *c.*7000 BC, to the Balkan peninsula and so to Europe as a whole, and subsequently beyond.

It is further postulated here that proto-Afro-Asiatic languages were spoken at about this time in the Levant. Among relevant early farming sites within this lobe are Jericho, Beidha, Tell Ramad, Tell Mureybat and Tell Abu Hureyra. The agricultural dispersal in this as in other instances would be in the direction of adjacent lands lying outside the nuclear farming area, namely in this case to south and west. So we might expect the proto-Afro-Asiatic languages to be propagated southwards, towards Arabia, and westwards, to Egypt and north-east Africa. Little is at present known about early farming sites in north Africa, and it cannot be claimed that there is any general evidence for the sort of 'wave of advance' propagation of farming of the kind that can be argued for south-eastern and central Europe. Indeed, Muzzolini (1989) lays emphasis upon local demographic processes, and would prefer to adduce local factors of the kind stressed for Europe by Zvelebil & Zvelebil (1988). But, as in the case of Europe, the cereal plants upon which the economy largely depended were imported species, and so in many cases were the domesticated animals which formed the pastoral basis.

It certainly cannot yet be documented that a language replacement accompanied the dispersal to Africa of these Near Eastern species. But the proposal perhaps becomes a more reasonable one if it can be admitted that some *general* cultural, historical or demographic factor must underlie the Afro-Asiatic linguistic unity. Agricultural dispersal is one of the few such simplifying hypotheses available, although, as in Europe, the ultimate outcome will inevitably have been the consequence of a whole series of different processes operating at different times.

In the case of Egypt this proposal harmonizes adequately with our present knowledge. For recent work has not confirmed earlier reports of very early cereal domestication in and near the Nile Valley (Wendorf *et al.* 1980). Farming in Egypt is first attested at a distinctly later date than in the Levant, and draws upon the same range of crops.

It should be noted however that Ruhlen thinks it unlikely that the Afro-Asiatic homeland was in the Near East. His objections are of a linguistic nature:

> According to what is known as the 'Age-Area' hypothesis in linguistics, the homeland for a family should be located in the area of greatest linguistic divergence. With respect to Indo-European, the great divergence of the Anatolian branch from the rest of Indo-European points to a homeland in Anatolia, as emphasized by Dolgopolsky, which matches the point of origin of the spread of agriculture into Europe through Greece and the Balkans. With respect to Afro-Asiatic, however, the

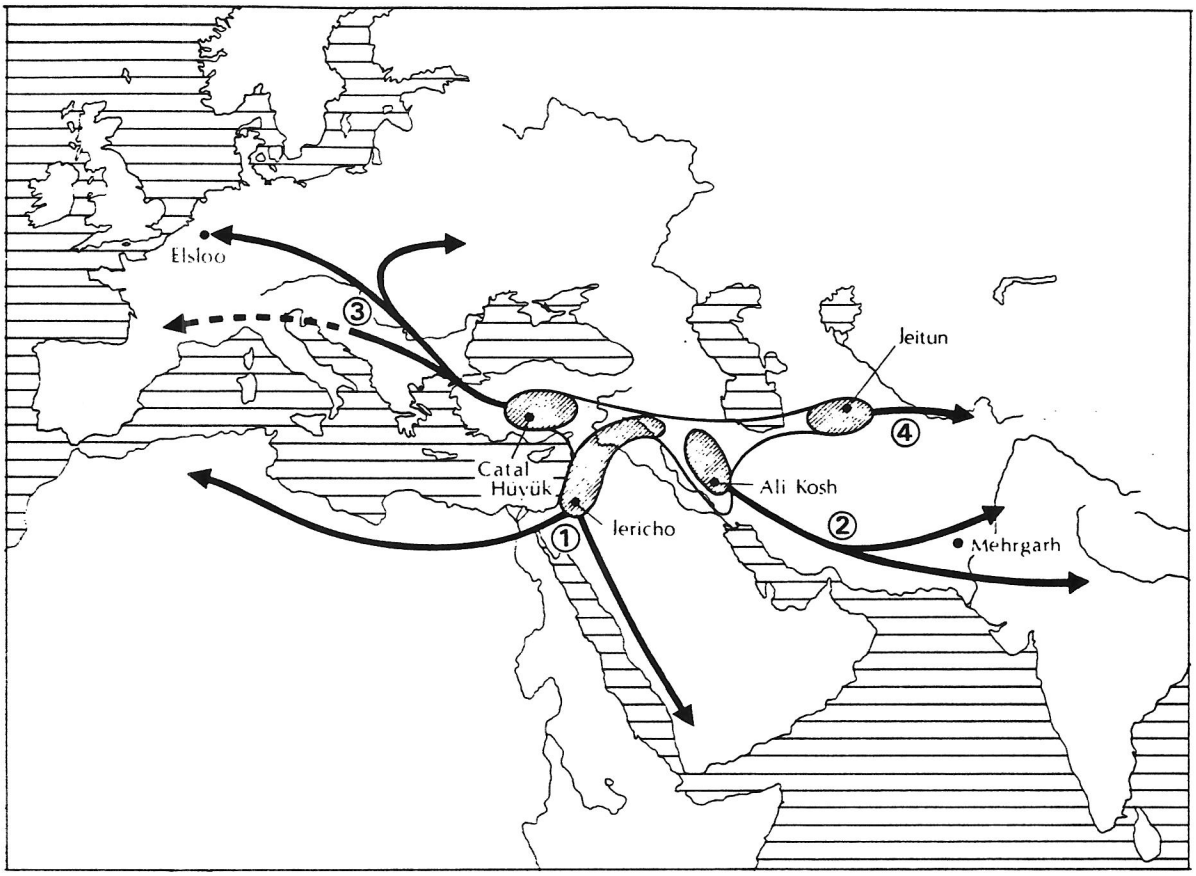


Figure 5. *Hypothetical application of the model seen in Fig. 4 to account for the distribution of the Nostratic macrofamily. Agricultural dispersals of related proto-languages originally located within the area where primary farming developed (hatched) are postulated as underlying the subsequent distributions of the (1) Afro-Asiatic (2) Elamo-Dravidian (3) Indo-European, and (4) Altaic language families.*

> centre of linguistic diversity would have to be located in Africa, especially if Omotic is the Anatolian of Afro-Asiatic, as some claim. But even if Omotic is just one of several coordinate branches, the fact remains that all of the branches are located in Africa, with the exception of Semitic which is found in both Africa and the Near East. Thus the linguistic evidence would speak for an expansion from Africa to the Near East rather than vice versa. (Ruhlen, pers.comm.)

This interesting observation would not harmonize with the scenario proposed here. Clearly there are several difficult issues to be reconciled before these proposals can be regarded as well-established.

The hypothesis of agricultural dispersal can also be applied to the South Asian languages of the Nostratic group. It can be argued that the early farming economy of Pakistan, as documented at Mehrgarh (Jarrige 1980), was the consequence of a dispersal from the south Zagros area, where very early farming is attested at such sites as Bus Mordeh and Ali Kosh, or from that part of the Iranian plateau nearby to the east. Of course the possibility of the local development of farming in West Pakistan, as Jarrige might prefer, should not be discounted. But at the moment the attested distribution of the wild prototypes for wheat and barley lies further west.

This process *could* have been responsible for the dispersal eastwards of a proto-Elamo-Dravidian language. (See also Zvelebil & Zvelebil 1990, 255; for the possible affinity of Elamite and the Dravidian languages, see McAlpin 1974; 1981.) Elamite of course continued to be spoken in Khuzistan for some time. The Dravidian languages, it may be suggested, came as a result of this process to be spoken in most of Pakistan and India. However they suffered a subsequent linguistic replacement in Pakistan and North India in a later episode of élite dominance which brought the Indo-Aryan branch of the Indo-European family to India. (This corresponds to Hypothesis B of the discussion offered earlier (Ren-

frew 1987, 197). This episode of élite dominance may have been as early as the *floruit* of the Indus civilization, and was probably associated with the development and spread of nomadic pastoralism.)

In a similar way, we can suggest that the earliest farming of the south Caucasus region was the result of a more localized farming dispersal from north Kurdistan (where Jarmo represents an appropriate early farming site) or from eastern Anatolia (where Çayönü may be cited). This dispersal may have been associated with the proto-Kartvelian language.

Finally it is proposed that the development of farming in Turkmenia may have been a significant point of departure for the early development of farming in central Asia, and for the initial dispersal there of the proto-Uralic and proto-Altaic language. Early farming sites such as Djeitun have been well-documented in Turkmenia (Masson & Sarianidi 1972). Only very much later, through subsequent episodes of élite dominance (mostly associated with further developments in mounted warfare and mounted nomadic pastoralism) did the Altaic languages reach their ultimate and very considerable expansion.

The Uralic languages (including Finno-Ugric) may have had a rather later origin than the five language families already briefly discussed. Their distribution is no doubt associated with the subsequent adaptation of the farming economy to the harsher climate of the north European plain, as Dolukhanov (1986) has argued, although it should be noted that this was a case of initial colonization, not of linguistic replacement. We are speaking here, then, of a Nostratic ancestor for the Uralic languages which would be related to proto-Indo-European.

These proposals would place all the early Nostratic languages or proto-languages discussed (with the exception of proto-Uralic, for which a later emergence is envisaged) within the nuclear zone of early farming in the Near East, very much within what Robert Braidwood well described as 'the hilly flanks of the fertile crescent', including east-central Anatolia, to which Turkmenia should possibly be added. This area, from the Levant northwards to Kurdistan and eastern Anatolia, and then south to the southern Zagros, is certainly itself a large region, but nothing like the extent of the subsequent Nostratic expansion.

At a date around 10,000 BC these proto-languages may already have existed as distinct languages or dialects. It would be possible to hypothesize an earlier, single Nostratic proto-language in this area if that were considered appropriate. Such a proto-language would have to be equated with the late Upper Palaeolithic cultures of the region, probably at a date subsequent to 15,000 BC. While an earlier date could also be suggested, that would begin to impinge upon the Gravettian proposal outlined earlier. At the same time, the present hypothesis might restrict the proto-Nostratic language to the regions adjacent to the fertile crescent just outlined.

It would be a mistake to suppose that the full range of variation observed could be covered within the scope of a single model. It could very well be that languages related to proto-Nostratic were spoken over a very much wider area than is here suggested, but that those which ultimately prevailed were indeed propagated through the agricultural dispersals discussed. There is no reason why the process of agricultural dispersal should not have taken place against the background of some pre-existing elements of linguistic unity.

It remains to be asked whether there is really sufficient evidence for the agricultural dispersals proposed above, beyond the Anatolian dispersal to Europe which has already been widely discussed and accepted (which is not to say that the proposed linguistic correlation can yet be regarded as established). And then it remains to consider whether there is any adequate evidence that such farming dispersals, if they occurred, did indeed have linguistic consequences like those hypothesized above.

There are undoubtedly many difficulties with this broad picture. How, for instance, do the Omotic and Chadic languages of the Afro-Asiatic group fit in, when wheat and barley play little significant part in the agriculture of the areas in question? The early use of sorghum and millet would be more relevant there. It is difficult to see the inception of agriculture in the eastern part of sub-Saharan Africa as part of the same dispersal that brought domestic cereals to Egypt and North Africa. If the general hypothesis is correct, then, there is certainly need of several subsidiary hypotheses to explain regional nonconformities such as this.

It should be noted, moreover, that the outline historical sketch offered above is closely dependent upon the precise definition of the Nostratic macrofamily. In this I have followed the pioneering work by Soviet scholars. But Merritt Ruhlen has pointed out to me that a very different outcome would result if one followed instead the grouping of language families brought together by Greenberg (1987, 259) in his 'Eurasiatic' macrofamily, where Eskimo-Aleut and Chukchi-Kamchatkan, among others, are seen as more closely related to Indo-European than is Afro-Asiatic. As Ruhlen puts it:

> To look for a homeland for classical Nostratic, which would include Afro-Asiatic but exclude Eskimo-Aleut, is to look for a homeland for a group that never existed. In historical linguistics it is absolutely imperative to work with valid linguistic taxa if the historical infer-

> ences are to have any chance of being valid. This in my view is the Achilles tendon of Nostratic work and the reason why Greenberg's Eurasiatic resembles but is distinct from Nostratic. (Ruhlen, pers.comm.)

This is a useful cautionary note, and a reminder that the proposals offered here will need to be reviewed as linguistic researches proceed.

Another point which makes one uneasy here is the position of the Dravidian languages. In the first place, the Elamo-Dravidian unity is a debatable one. And it is notable that the Dravidian languages in recent times have been spoken by the hunter-gatherer communities of central and southern India. At the same time, however, it should not be overlooked that hunter-gatherer groups may in some cases adopt the language of their farming neighbours without adopting agriculture (Ehret 1988).

Nor does this discussion deal with such early non-Semitic (and mainly non-Nostratic) languages in the Near East and Anatolia as Sumerian, Hattic or Hurrian. For the Sumerian case I have an inkling that there is a special link with the adaptation to the environment of the lower Tigris and Euphrates which we have come to associate with the Marsh Arabs. Their characteristic reed architecture is already clearly depicted on many Sumerian cylinder seals. Early Sumerian may have been the language spoken by a group which already in late Upper Palaeolithic times was well adapted to this environment. It may have been one which proved particularly resistant to the initial impact of a farming economy, in the same manner that Zvelebil & Zvelebil (1988) have proposed for some of the non-Indo-European languages of western Europe which survived into Roman times.

As for Hattic, Ivanov (1985) has proposed a relationship with proto-North Caucasian. The North Caucasian languages occupy an isolated block enclosed by lands where Nostratic languages were spoken. And Diakonoff & Starostin (1988) have suggested that Hurrian (along with the later Urartian) may be related to the East Caucasian sub-group of the North Caucasian languages. So we may envisage an initial, rather broader distribution for the early North Caucasian languages, perhaps including eastern Anatolia and beyond, which subsequently became more restricted.

In the foregoing section I have suggested that the phenomenon of agricultural dispersal may be a key one when we set out to consider, at world level, the historical reality behind the distribution of language families. This is a point also made by Shnirelman (1989). The speculations here concerning the proposed Nostratic language family are now concluded. However it will be appropriate to situate this discussion within the context of a geographically even broader picture. For there is a further significant development in current linguistics which again may have interesting archaeological implications.

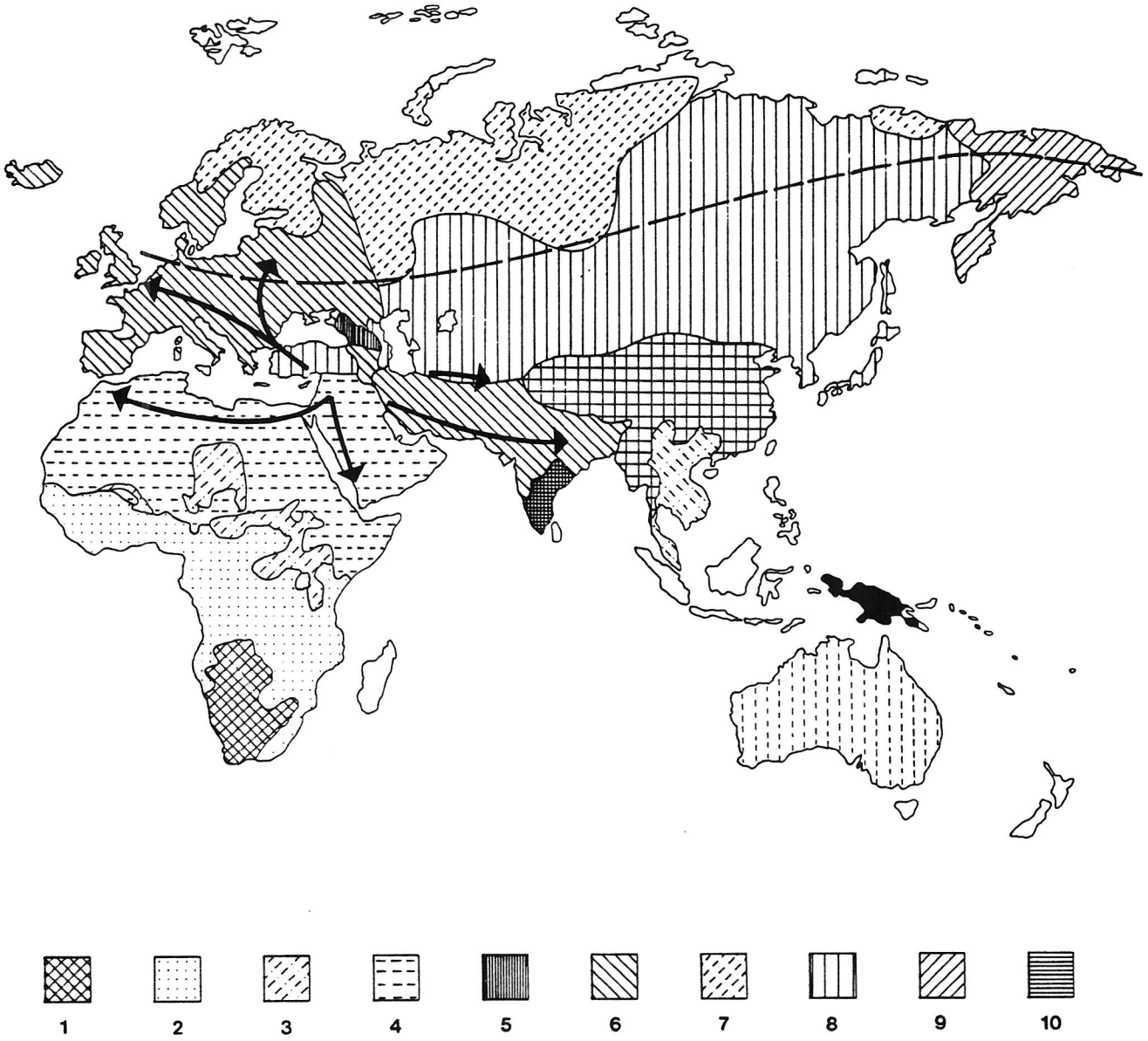
World Language Families

The very scale of the linguistic units involved (Altaic, Afro-Asiatic, etc.) when we are discussing the Nostratic hypothesis is at first sight rather daunting. The archaeologist, used to the much more restricted cultural units of later prehistoric times, certainly finds it difficult to conceive of social regularities and processes operating on this scale. It is worth noting in passing that the areal extent of the distribution of the Indo-European languages is very nearly equivalent to that of the Roman Empire at its maximal expansion, if the territories conquered by Alexander the Great and briefly ruled by his successors are added. No single political authority, not Alexander, not Tamburlaine, not even Stalin, has ever governed so vast a political unit as the sum of the territories where the Nostratic languages were spoken, neither in early historical times nor subsequently. It is difficult, then, to account for such vast unities in terms of the migrationist approach of traditional prehistoric archaeology which, as Gordon Childe aptly characterized it (Childe 1958, 70): 'aimed at distilling from archaeological remains a preliterate substitute for the conventional politico-military history, with cultures instead of statesmen as actors, and migrations in place of battles'. That is one reason why the eminently political model of élite dominance seems an inappropriate one in many cases, and why broader underlying economic or ecological processes (in the analysis of which Childe himself was an innovator) appear more relevant.

The great macrofamily or linguistic phylum represented by the Nostratic languages becomes at any rate slightly less astonishing in its scale when it is considered in the context of further new linguistic work, which again is of the highest interest to the prehistoric archaeologist or to the student of early human history. Once again this work is novel and controversial, having met a very mixed reception among competent linguists. But its consideration, even if it were later shown to be unsound, opens interesting avenues. If the work is substantiated by further linguistic research it will certainly prove to be something of which archaeologists will have to take note.

The work in question is the division of the great diversity of the world's languages into fewer than twenty very much larger units (superfamilies, macrofamilies, phyla) by the American comparative linguist Joseph Greenberg, conveniently summarized and extended in Merritt Ruhlen's recent *Guide to the World's Languages* (Ruhlen 1987; see also Ruhlen 1991).

In 1963 Greenberg proposed that the great multiplicity of the African languages could be simplified by



classifying them into just four over-arching macrofamilies. These are the Afro-Asiatic, which we have already discussed, the Niger-Kordofanian (which includes the Bantu languages), the Nilo-Saharan, and the Khoisan (including the languages of such hunter-gatherers as the Bushmen and the Hadza). The methodology employed proved controversial among linguists, depending upon what Greenberg terms 'multilateral comparison', that is to say on lexical similarities studied in a number of languages at the same time. Previously most linguistic comparisons had been made two at a time, but Greenberg claims to reach much greater time depths with his multiple comparisons. Less satisfactory, however, in the eyes of many historical linguists, is his failure to establish the regular phonological correspondences employed by the comparative method, or to make the standard morphological comparisons. Nor does he claim to reconstruct the original proto-language which he would, nonetheless, posit as the ancestor of each language family. He is content to base his work upon contemporary lexical comparisons alone. Despite these apparent limitations, his African classification has, in fact, been followed by most comparative linguists, and appears now to have the status of an accepted and standard classificatory organization for the African languages.

More recently he has classified the languages of

Figure 6. *The principal language macrofamilies of the world (classified following Ruhlen 1987). The broken line indicates the approximate northern limit of human dispersals during the Pleistocene period. (Arrows indicate the hypothetical Nostratic agricultural dispersals shown on Fig. 5.) The language macrofamilies are as follows: 1. Khoisan; 2. Niger-Kordofanian; 3. Nilo-Saharan; 4. Afro-Asiatic; 5. Caucasian; 6. Indo-European; 7. Uralic-Yukaghir; 8. Altaic; 9. Chukchi-Kamchatkan; 10. Eskimo-Aleut; 11. Dravidian; 12. Sino-Tibetan; 13. Austric; 14. Indo-Pacific; 15. Australian; 16. Na-Dene; 17. Amerind.*

the Americas into just three major superfamilies: Eskimo-Aleut, Na-Dene, and Amerind (Greenberg 1987). The last of these categories embraces a great variety of the languages of the Americas, not hitherto seen as related.

He and other linguists, most notably Ruhlen, have employed comparable techniques to classify *all* the world's languages into just 17 major macrofamilies (leaving out the recent creole and pidgin languages, and not counting, within the 17, five specific language isolates: Basque, Burushaski, Ket, Gilyak and Nahali).

We have already touched on most of these 17 families in the course of this article: four in Africa; three in the Americas; Caucasian; and the other languages within Illich-Svitych's Nostratic group (Indo-European, Uralic-Yukaghir, Altaic and Dravidian). There are five more: Chukchi-Kamchatkan; Sino-Tibetan; Austric (including Austronesian); Indo-Pacific; and Australian. In offering this synopsis I am following the list given by Ruhlen (1987, 29): most linguists, including Greenberg, would however prefer to subdivide 'Caucasian' and thus distinguish between the North Caucasian and the South Caucasian (Kartvelian).

Once again it seems a worthwhile undertaking to consider what might be the historical or archaeological background to this distribution, while bearing in mind

that the classification is not yet well established. As Anna Morpurgo Davies (1989, 167), one of Greenberg's more lucid critics, puts it: 'We do not yet know whether superfamilies outlined in this way have the same properties as the families established with the standard comparative method. If they do not, there is a serious risk that the whole concept of superfamily is vacuous.' Without discounting this reasonable and prudent criticism I have elsewhere (Renfrew, in press) set out to show how the spatial patterning proposed by Greenberg and Ruhlen might be explained using a series of simplifying models which relate language change to social and demographic phenomena. They involve considerations of initial colonization (in both Pleistocene and post-Pleistocene times) and of linguistic replacement. In only a few cases (the Altaic languages and the Indo-Aryan branch of the Indo-European family) was it found necessary to use the élite dominance model for linguistic replacement. In other cases the agricultural dispersal model seems more appropriate.

The cases of initial colonization can be separated into two groups: those in temperate or equatorial zones, where the colonization took place in Pleistocene times, and those in the extreme north, where that colonization seems to have occurred after the end of the last Ice Age. We have already referred to one such northern instance (the Uralic-Yukaghir). The three others, I suggest, are the Eskimo-Aleut, the Na-Dene and the Chukchi-Kamchatkan.

In the map reproduced here (Fig. 6) the basic division of the world's languages is shown as proposed by Ruhlen. On it is superimposed the line of northernmost settlement during the Pleistocene period. It will be seen that the four language families just cited lie to the north of this line.

Six language families on that map are probably in their distribution mainly the result of the process of initial colonization during the Pleistocene period (and subsequently also in the case of the Austronesian languages). These six families are the Khoisan, the Nilo-Saharan, the Austric, the Australian, the Indo-Pacific and the Amerind.

In this article, in view of the discussion in the foregoing section, I should like to stress the contribution to the world distribution of languages made by processes of agricultural dispersal. We have already discussed the three or four major instances of possible agricultural dispersal within the Nostratic superfamily: the Indo-European, the Afro-Asiatic, the Elamo-Dravidian and also the Altaic (although in this case the major phase of expansion came later, with the episode of élite dominance associated with mounted warfare). We have touched also upon the Bantu expansion, responsible for the very wide distribution of the Niger-Kordofanian languages. The Sino-Tibetan case, however, has not yet been discussed. Indeed it contains a complexity, since the earliest agriculture in China, associated first with millet and then with wheat, has generally been thought to be in the upper valley of the Yellow River, associated with the Banpo culture. More recently, however, convincing evidence has emerged for a very early focus of rice cultivation on the coast at the site of Hemudu (Chang 1983; Pearson 1983). This early focus of rice cultivation may prove to be part of a wider one within south-east Asia, also associated no doubt with the distribution of some of the Austric languages. We seem, then, to have two agricultural dispersals potentially associated with the distribution of the Sino-Tibetan languages, and it is not to be excluded that the present extent of these languages reflects also some later episodes of élite dominance, perhaps associated with the formation of the Chinese state.

Of course the more restricted loci originally occupied by the proto-languages for each of these families will themselves need to be examined in the light of earlier processes, in many cases processes of initial colonization. But there is the exciting prospect that much of the patterning in the world's diversity of languages may be explained, at any rate as a first approximation, in such a way as this. Within each group there would be needed a full analysis of the comparative linguistics, and then a further discussion, in order to offer explanations at a more satisfyingly detailed level. But at least in a few years we can expect prehistoric archaeology, supported by radiocarbon dating, to offer us a very clear picture of the various processes of initial colonization and of agricultural dispersal. The proposed linguistic correlations will no doubt remain very much more hypothetical, the subject of considerable debate.

All of this discussion has proceeded so far without taking account of the evidence now becoming available from molecular biology. From it we can expect significant new insights, as well as arguments for or against the suggestions made above.

The Genetic Approach

Only three potentially independent sources of information are available for the early human past, for times and places before the existence of written documents. *Archaeology* naturally offers the opportunity of making inferences from the material remains generated by the humans in question: these inferences can be specific in place and in time. *Historical linguistics* has available to it a body of data which, as we have seen, is not directly related to the findings of archaeology and is surprisingly difficult to correlate with them. Indeed, the difficulties in devising an appropriate methodology for establish-

ing such relationships form the underlying subject matter of the present article. *Historical genetics*, if one may by this term link two hitherto disparate fields of study, today offers the opportunity of reconstructing with some precision the ancestral relationships between living human groups. Moreover techniques are now being developed which are likely to allow the genetic approach to be used on ancient material also (Pääbo 1985). Such relationships, it should be noted, would be established without any necessary reference to the two earlier classes of evidence, the archaeological and the linguistic. They will thus, if correctly interpreted, be in a position to shed new light upon the archaeological and linguistic questions which we have been discussing.

The novelty of the present situation could easily be overlooked, for the standard, now classical techniques of biological anthropology have over the last century presented a whole series of insights into human evolution, based upon the morphological study of skeletal remains (e.g. Le Gros Clark 1964).

But these techniques, appropriate to the major morphological distinctions which differentiate between, for instance, *Australopithecus* and *Homo erectus*, have been less successful in giving insights into the historical origins of the very much more restricted morphological diversity which is seen within the single species *Homo sapiens sapiens*. That species includes all the humans alive in the world today, and over the past twenty thousand years and more. No modern anthropologist can feel other than uneasy at works (e.g. Coon 1962) which purport, on the basis of classical physical anthropological (i.e. morphological) methods, to give a historical account of the present state of human physical diversity. The explanation in this way of the supposed differences between modern human 'races' has not been successful. No archaeologist, likewise, can be other than uncomfortable at the outcome when the same morphological techniques, usually applied to cranial measurements, are used to give some purported account of the physical (and in that sense ancestral) relationships between different cultural groups in the past (e.g. Angel 1971; Nemeskeri 1956; Vencl 1986).

The application of molecular genetic techniques is likely to transform this unhappy situation. Already, much biological information is available, mainly from studies of blood groupings undertaken by the now classic techniques of the molecular biologist (e.g. Cavalli-Sforza *et al.* 1988). The publication by Cavalli-Sforza of his remarkable *History and Geography of Human Genes* (Cavalli-Sforza, in press) will offer an impressively full overview of the information available for gene frequencies obtained mainly through blood group studies. But a word of caution is needed here, since the paths of inference from contemporary genetic data to historical reconstruction are not direct. They are perhaps as difficult as those from archaeology to historical linguistics, and there is a risk of premature conclusions. Thus Harding & Sokal (1988) have criticized the linguistic conclusions reached by Cavalli-Sforza and his colleagues from their genetic data, arguing that 'the resulting classification largely reflects geographical propinquity rather than linguistic origins'. A critical position has been taken also by Bateman *et al.* (1990).

It is the more recent development of DNA studies which now opens the way to a much finer, more tightly focused approach. Here the relationships between living individuals, or between living groups, can be assessed, not by gross molecular features such as blood groups but, at what is effectively the genotype level, by establishing the actual sequence of bases within the nucleic acid chain which constitutes the genetic information passed on from individual to individual across the generations (e.g. Hill *et al.* 1987; Stoneking & Cann 1989).

This approach will permit much more precise and reliable measures of genetic distance between individuals and groups. To convert this genetic information into statements about the past will become the special field of historical genetics. It will not directly tell us about human culture or about language. But it will give unambiguous information about descent, in physical, ancestral terms. Many of the models which we have been using in this paper carry with them implications about ancestry and descent. It cannot be doubted therefore that the emerging field of historical genetics will have much to offer that bears upon these hypotheses and controversies, even if the problems of interpretation will not easily be overcome.

Conclusions

In the preceding discussion I have tried to set out, if inadequately, the interesting implications for prehistoric archaeology of some recent and still rather controversial work in the field of historical linguistics. Neither the Nostratic hypothesis nor the 'superfamily' classifications of Greenberg can be considered to be acceptable yet in the eyes of many linguists. Nor is it for an archaeologist without expertise in the field in question to try to offer a verdict.

What one can do, however, is begin to consider the implications for archaeology if these two very important, simplifying approaches were indeed to prove acceptable. (It should be noted that Greenberg is broadly in agreement with the Nostratic approach, although his own equivalent 'Eurasiatic' group is not identically defined.) The opportunity for some fairly direct correspondences between the archaeology and the linguistics

would then arise. I have commented on a number of these, and the current pace of research in these fields ensures that more such observations will be made.

Not all of the proposals ventured in this article are likely to be correct, and I offer them only to indicate the vast scope of the field, its considerable inherent interest, and the rewards in terms of the new understanding and the satisfying synthesis which may result.

At least some of these speculations are capable of being tested against independent lines of evidence. For instance, the rather simplistic view of agricultural dispersals taken here would give rise to a number of predictions of a demographic nature which should be open to testing in the light of the emerging molecular genetic data.

Why all of this should be thought to matter is another question entirely. It could reasonably be argued that there could be few more profitless fields of enquiry than the nature of languages which may have been spoken millennia ago by people who had no means of recording them: they must always remain hypothetical. But I feel that there are two coherent arguments against such a view. In the first place, the 10,000 or so languages spoken in the world today offer a picture of such apparent complexity that any attempt at discerning some patterning in it, and then of explaining that patterning, must be of value.

The second reason is the close relationship between language and ethnicity. No one can doubt the force with which ethnic adherence is maintained over much of the world today. Ernest Gellner (1983) has shown how the particular form of ethnicity associated with the nation state is of relatively recent origin. But underlying it are ethnicities - deeply felt, mainly inherited group affiliations - which have been and are as crucial to the construction of personal identity in much of the modern world as are citizenship and nationhood. Although we do not yet understand very well quite why there are so many (or, alternatively, so few) different languages in the world, we do already have an insight that language diversity is related in various ways to identity and to ethnic diversity, as George Steiner (1975) reminds us in the quotation set at the head of this article. These are issues with which the archaeologist, bogged down still by the cumbersome concept of archaeological 'cultures', has not yet learnt to deal.

For that reason I feel that the matters discussed here are important ones. Speculation about them will lead, I hope, to counter-speculation, and in the end to the formulation of well-framed hypotheses. In a couple of decades, I predict, we shall see emerging a new synthesis between historical linguistics, prehistoric archaeology and molecular genetics. Whether it will have any resemblances with the foregoing remains to be seen.

Postscript: Back to Monogenesis?

The reader may have felt, on perusing the foregoing, that the argument could be extended further, towards what many might consider a *reductio ad absurdum*. For would not the scholar's progression towards ever larger (and more hypothetical) linguistic units - from dialect (e.g. Cockney), to language (e.g. English), to language group (e.g. Germanic), to language family (e.g. Indo-European), to macrofamily (e.g. Nostratic) - inevitably lead to that logical larger unit where all the languages of the world would be classified together in a single taxon? And would that not imply, following the genetic method, that all were descended from a single, hypothetical, early world language: 'proto-World'? And how would that differ from that old and ill-substantiated speculation, the monogenesis of human language (Trombetti 1905), which achieved such a degree of vacuity towards the end of the last century that in 1896 it was actually pronounced a proscribed topic by the Société de Linguistique de Paris?

Such intimations would be fully justified. For several scholars have now claimed to recognise cognate (i.e. genetically related) words between macrofamilies in circumstances where borrowing is not thought to have taken place. Thus Ruhlen (1990, 92) has listed cognate words between the Amerind and the Nostratic/Eurasiatic macrofamilies. And Kaiser & Shevoroshkin (1988, 310) report the proposed addition to the Nostratic macrofamily of two further language groups, Niger-Kordofanian and Nilo-Saharan, which have hitherto been considered entirely separate (Greenberg 1963). Indeed, the monogenetic nettle has been firmly grasped by Ruhlen (1991, 16) who, with John Bengtson, has proposed thirty etymologies connecting all of the world's language families, and has offered documentation for two of them, namely for TIK ('finger, one') and PAL ('two').

One's first reaction is not only that this may be going too far, but that the formulation of such conclusions by the advocates of 'lumping' rather than 'splitting' in the field of linguistic taxonomy could provoke reservations about the good judgement of these 'lumpers'. For we are talking here of cognates that must extend back at least 20,000 years (when proto-Amerind must have separated from proto-Eurasiatic), if not further. Yet before dismissing such arguments out of hand, it should be remembered that probably the majority of archaeologists and biological anthropologists now favour an African origin for all the branches of our own species, *Homo sapiens sapiens*, in what amounts precisely to an argument for monogenesis (Stringer 1989). Admittedly the argument, 'the fossil alternative to Eden', for several lines of descent from the earlier *Homo erectus* has

also been re-asserted (Wolpoff 1989), in what is, of course, an advocacy of polygenesis. But the genetic evidence has been re-examined recently and pronounced to favour the monogenesis case (Cavalli-Sforza *et al.* 1988). And data from molecular genetics, specifically for mitochondrial DNA (Stoneking & Cann 1989), have promoted speculation about the possibility of an original 'mitochondrial mother', an African Eve.

These terms may perhaps over-simplify the genetic arguments, but the strong likelihood of an African origin, around 100,000 years ago, for the entire human species must now be recognized. Is it then inconceivable that there might be preserved within the modern diversity of human speech some echoes of words from the early dialects then spoken in a single hypothetical ancestral language, which might well be termed 'proto-World'? Such speculations take one further than I myself would wish to go at present. But undoubtedly they bring us back towards that earliest recorded formulation of the monogenesis position, surely the most authoritative in the field of historical linguistics - where else but in Genesis? - with which I began this paper: 'And the whole earth was of one language and of one speech ...' After Babel, things became more complicated.

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